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COMMENTS ON LERMAN AND IWATA (1996)

JOSEPH E. SPRADLIN
SCHIEFELBUSCH INSTITUTE FOR LIFE SPAN STUDIES
UNIVERSITY OF KANSAS

Lerman and Iwata's article represents a departure from typical *JABA* publication practices. First, it is a review and discussion article, and second, it is quite lengthy. However, it serves as a model for a type of article that is needed in the field of applied behavior analysis. It presents a comprehensive, well-organized review of basic and applied research on a major topic in behavior analysis. It analyzes the literature on that topic in a critical yet even-handed manner. It notes where there is a lack of information and suggests specific research. Finally, there are suggestions based on the review and analysis that, if followed, should lead to improved behavioral treatment.

One reviewer of Lerman and Iwata's article wrote, "This is a comprehensive and well-organized review that will guide applied research for years to come." It is precisely because it is likely to have a powerful effect on future research and clinical practice that it should receive critical attention.

The article reviews the variables applied during acquisition and maintenance that increase resistance to extinction. Some of these are intermittent reinforcement, delay in reinforcement, and variable stimulus conditions. Conversely, continuous reinforcement, immediate reinforcement, and constant stimulus conditions are associated with less resistance to extinction. In general, the experimental literature on these variables is consistent. The authors suggest that, if one seeks to produce durable behavior that will persist during periods of nonreinforcement, conditions that increase resistance to extinction should be introduced during training. This suggestion is justified by the research literature. The authors also make a second suggestion, the gist of which is best presented in the following quote:

> In the case of problem behavior, for example, treatment efficacy might be enhanced if caregivers or therapists alter several variables prior to extinction. Specifically, the therapist could switch from a PRF to a CRF schedule, shorten the latency between the occurrence of a response and reinforcer delivery, alter the reinforcement magnitude (the direction may depend on the reinforcer and how this variable is modified), and eliminate any variation in the conditions associated with reinforcement. (p. 372)

Whether the suggested procedures will reduce resistance to extinction is an empirical question. The authors believe that they will. Introducing conditions associated with reduced resistance during maintenance (prior to introducing extinction) is appealing. The rationale is congruent with behavior-analytic views that the effects of behavioral treatments are reversible. And, reduced resistance to extinction would have tremendous implications for treatment of problem behavior. However, there is little evidence for the hypothesis that introducing conditions associated with lowered resistance to extinction prior to introducing extinction will result in lowered resistance. The paucity of evidence could be a function of researchers overlooking this possibility. Or it could be a function

Address correspondence to Joseph E. Spradlin, 3144 Campfire Drive, Lawrence, Kansas 66049.

of the failure of such procedures to reduce resistance to extinction. Consider one specific example. The superiority of intermittent reinforcement over continuous reinforcement in producing behavior that will persist when reinforcement is discontinued is probably one of the oldest and best established principles in the literature. Because continuous reinforcement is associated with fewer responses during extinction, numerous writers have considered the possibility that introducing a period of continuous reinforcement after intermittent reinforcement would reduce the number of responses that occur during extinction (Keller, 1940; LeBlanc, 1970; Lerman, Iwata, Shore, & Kahng, 1996; Likely, 1958; Spradlin, 1962). However, as noted by Lerman and Iwata, only a small number studies concerning the effect of presenting continuous reinforcement after intermittent reinforcement have been published, and these have reported mixed results. Is the lack of studies showing reduced responses during extinction after a period of continuous reinforcement merely an oversight of researchers, or is there a lack of reported studies because the procedure has been tried, but has been ineffective?

Let us consider each of these possibilities. Lerman and Iwata's review notes studies conducted from 1961 to 1996 that have investigated the effect of a period of continuous reinforcement on extinction. Even earlier, Keller (1940) reported the effects on extinction when following an intermittent reinforcement schedule with a continuous reinforcement schedule, compared to following a continuous schedule with an intermittent schedule. There was no difference in the overall rate of responses or number of responses until the subjects reached criterion for extinction. Clearly, researchers have not overlooked the possibility that following intermittent reinforcement with continuous reinforcement might reduce resistance to extinction. If, after over 50 years of consideration, a consistent effect of following intermittent reinforcement with continuous reinforcement hasn't been demonstrated, it seems likely that no robust effect exists. Why isn't there such a consensus in the published literature? The answer may rest in publication practices. When an experimenter investigates a variable and finds no effect, the results are not likely to be submitted for publication. If they are submitted, they are less likely to be published than studies demonstrating an effect. So it seems likely that the proportion of studies published will be much higher for studies in which an apparent effect is demonstrated than for studies of equal or superior quality that show no effect.

It should be clear from the comments above that I question whether introducing conditions associated with low resistance to extinction after conditions associated with high resistance to extinction will result in fewer responses during extinction. Whether introducing such conditions reduces resistance to extinction is an empirical question. Hence, one might hope that it would be expeditiously answered in the literature. However, unless there is a powerful effect, the current publication practices make a prompt answer unlikely. Perhaps, the likelihood of an expeditious research-based answer would be increased if *JABA* encouraged submissions of direct and systematic replications of studies that report effects and evaluated these replications for publication on the basis of the quality of designs and measurement, independent of whether their outcomes are positive or negative. If such replications are not submitted and published, the Lerman and Iwata article may send researchers on an indefinitely long and fruitless search for effects that don't exist.

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